denser is placed in a vacuum, then, so far as I can see, Prof. Lodge's principle must lead to the conclusion that the difference of potential between the plates of the condenser is proportional to the Peltier effect; but if this is so, it is quite easy to show by the second law of thermodynamics that if the system is regarded as a heat-engine, the Peltier effect cannot vanish except at the zero of absolute temperature.

On the other points mentioned by Prof. Lodge in his letter, there does not seem sufficient difference of opinion between us to make it worth while discussing them.

In conclusion, let me assure Prof. Lodge that I am thoroughly in sympathy with the view that the consideration of the behaviour of the medium in the electric field is absolutely essential. I do not think there is anything inconsistent with this in the paragraph he quotes, which was intended to express what is well known to have been the opinion of Maxwell himself—that the key to the secret of electricity would be found in the "vacuum" tube. THE REVIEWER.

The Physics of the Sub-oceanic Crust.

IN your article on the above subject in NATURE of November 21 (p. 54), the important proposition that the earth's crust rests on a liquid layer is once more brought to the front. The question reaches to the very basis of geology, but, like most of those of real importance, is not now recognized by the Society which occupies apartments in Burlington House, rent free, for the purpose of forwarding the study of geology.

Nothing is more obvious to the geological student than that enormous thicknesses of strata have been formed at practically one level. We do not find that, when a thousand feet of sediment has been deposited under water, the deposition began in water which was 1000 feet deep, and went on gradually lessening the depth until the sea or lake was filled up; but we do find, as in the coal-measures, that the entire 1000 feet was deposited in most uniformly shallow water; that therefore the crust of the earth must have sagged foot by foot as additional feet of burdens were laid upon it. Deltas that have not yet been bottomed show hundreds of feet of silt, every yard of which was deposited at only a few feet from the surface level of the water; estuaries and river valleys slowly sink where there is sedimentation; ice-caps tell of accumulation accompanied by depression and submergence, and re-elevation when the burden is melted and dissipated; coral formations and submergence are regarded as well-nigh inseparable, and even lava-flows flowing on to a plain have sunk its level in a degree corresponding with their mass. Where there is fifty or a thousand feet of piled-up lava-sheets you may look for a fault of like amount on its flanks, like that which, still unsuspected by geologists, cuts the Isle of Mull in half. Whether we look at the past or the present, we seem to see evidence of a crust resting in equilibrium on a liquid layer, and sensitive to even apparently insignificant readjustments of its weight. And if the crust did not respond to, and make room for, the burdens laid upon it by the removal of disintegrated land and its redeposition as silt under water, would not the seas be choked for miles round every coast? The abrading action of the waves cuts down the land, be it high or low, to a dead uniform level, and sooner or later it must become first beach, and then sea-bottom. There it is covered with silt or sea-weed, and is no longer abraded, and would, therefore, form great level tracts, instead of almost uniformly shelving coasts, unless it yielded *pari passu* to the increasing weight of sediment and water. The immediate effect of cutting down cliffs, say of 100 feet in height, and removing them in solution or by wave action, is to relieve the pressure at their base; and I claim that, wherever I have excavated for the purposes of collecting under such conditions, I have found a decided slope inwards away from the sea, if the strata were at all horizontal, no matter what direction their general inclination might be at a distance from the sea margin. But on the beach, a little way from the base of the cliffs, the slope is, on the contrary, towards the sea, and whatever the general inclination may be, we see the harder ledges, even if but a few inches thick, sloping away into deeper and deeper water until lost to view; and if you choose to follow them and dredge, you trace them tending downwards into yet deeper water. This appears to me to be simply because the relief from pressure has made the beach-line the crown of a slight arch, and an arch that continues to grow and travel, else how could we collect day after day and year after year, on the same spots, such as Eastware or Bracklesham Bays, fresh crops of fossils after every tide? I have hundreds of times picked up every vestige of a fossil on perhaps an acre of Eocene or Gault, yet a couple of tides have removed so appreciable a layer that the area has appeared studded with fresh specimens that were twenty-four hours previously wholly covered and concealed under matrix. Yet this ceaseless waste does not lower the level of the beach as it ought to.

And if such slight displacements as result from coast denudation have so appreciable an effect, what must take place in ocean, if subsidence is going on, and the weight of water on the increase? Darwin saw that the vast rise of land, which he so graphically describes in South America, must have been accompanied by a corresponding depression in the bordering oceans; and in turning his pages you almost expect to come on the view that depression in the Pacific must be the cause of the upheaval of the coast-line. It only wanted the liquid layer theory to make the dependence of one on the other obvious. No general rise of land has, or ever can, take place, under the overwhelming pressure of the great ocean depths, and oceans are thus permanent; the struggle is confined to whether the liquid layer shall overcome lateral resistance and find relief along the coast-lines, which are the nearest lines of least resistance, and already weakened by abrasion, forming coast ranges, or rending the crust, and pouring over thousands of square miles from fissure eruptions; or whether it shall overcome vertical resistance, and raise the beds of shallower ocean eventually, perhaps, into land.

Thus the tendency, as noticed by the writer of your article, is for deep oceans to become deeper, under pressure which may increase but never relaxes, and for mountain-chains to grow into higher peaks, the more weight is lessened by valleys being cut up and denuded.

This theory accounts for innumerable facts in the physics of the earth which space would not permit me to enter on, and is, so far as I know, opposed to none.

J. STARKIE GARDNER.

Area of the Land and Depths of the Oceans in Former Periods.

IN a letter to NATURE (p. 54), entitled "Physics of the Sub-oceanic Crust," by my friend, Mr. Jukes-Browne, the following passage occurs:—

"We are at liberty to imagine a time when there was much more land than there is at present, and when all the oceans were comparatively shallow."

I wish to point out that such a condition of things could not obtain if the bulk of the ocean water was the same as now. To get more land, the ocean would have to be *deeper* than now, not shallower. An easy way of conceiving the effect of shallowing the oceans is to mentally lift up the present ocean-floors, the result being an overflow of water and decrease of land area. The only possible way of shallowing the oceans and increasing the area of the land would be to make the ocean-floors perfectly flat, and to surround the continents with vertical walls of rock—in fact, to make the oceans into docks, which nevertheless would exceed two miles in depth.

I pointed out this geometrical fact in "Oceans and Continents"[1]—an article which has provided some of the stock arguments against their fixity. If, therefore, theorists feel it necessary that the land areas should be greater, and the oceans shallower, in former ages, they are bound at the same time to provide some means of decreasing the bulk of the ocean waters. This seems difficult, as other theorists tell us that the amount of water on the globe goes on decreasing, being used up in the hydration of the crust of the earth, and point to the condition of things on the moon as the final stage of our planetary existence.

T. MELLARD READE.

Park Corner, Blundellsands, near Liverpool,
November 23.

Distribution of Animals and Plants by Ocean Currents.

SOUS ce titre, vous donniez naguère (vol. xxxviii. p. 245) une correspondance de M. A. W. Buckland concernant divers phénomènes observés à Port-Elisabeth, dans l'Afrique du Sud. Entre autres choses il y était relaté que, vers la fin de l'année 1886, un fruit analogue à celui du cocotier avait été porté par la mer sur le rivage de Port-Elisabeth en même temps que des quantités considérables de pumites ou pierres-ponces.

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[1] *Geological Magazine*, 1880, p. 389; also, see letter in same magazine, 1881, p. 335, headed "Subsidence and Elevation."

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Le fruit ramassé par un *boy*, "*Il* y porte la dent, fait la grimace. . . . Le moindre ducaton serait bien mieux *son* affaire." Notre *boy* se décide dès lors à porter le fruit au jardinier de "North End Park." Le végétal confié à la terre poussa et donna un arbre, *Barringtonia speciosa*, qui avait atteint 4 pieds de hauteur vers le milieu de l'année 1888.

M. A. W. Buckland émettait l'hypothèse que fruit et pumites, comme aussi quelques poissons et serpents appartenant à des espèces jusque-là inconnues dans le pays, et arrivés en même temps, provenaient des parages de la Sonde, et, à la suite de la grande éruption de Krakatoa en 1883, avaient été portés par les flots jusque sur les rivages de la côte Sud-Africaine.

Il n'y a plus à douter, je crois, de la provenance des pumites. Je n'ai rien à dire au sujet des poissons et serpents. Mais pour ce qui est du fruit de *Barringtonia speciosa*, il me semble qu'on pourrait lui donner une autre origine ou point de départ, et diminuer ainsi de beaucoup la durée de sa traversée sur l'océan.

L'arbre *Barringtonia speciosa* croît, en effet, à Madagascar, où je l'ai vu à Tamatave, sur les bords de la mer. Il ne serait donc point du tout improbable que le fruit porté par les flots à Port-Élisabeth provint de la grande île Africaine. En même temps que je signalais l'arrivée sur nos plages Malgaches des pumites de Krakatoa, en Septembre 1884 et en Février 1885 (*Cosmos*, nouvelle série, No. 12, p. 320), j'envoyais en Europe divers spécimens de ces pumites ramassés sur la plage de Tamatave. Parmi les spécimens adressés à la Société Nationale d'Acclimatation de France s'en trouvait un dans lequel s'était logé une partie de végétal,—une fleur, si je ne me trompe, d'une espèce de *Terminalia*, qui croît aussi à Tamatave sur les bords de la mer (*Bulletin de la Société Nationale d'Acclimatation de France*, Décembre 1884, p. 983).

Un fruit de *Barringtonia speciosa* arbre qui, comme je l'ai fait remarquer, croît au bord de la mer sur la côte orientale de Madagascar, a très bien pu, de même, prendre "passage" sur une pumite ou un banc de pumites atterrées sur la plage Malgache ; puis, à la première haute marée, avoir cinglé sur ce "transport" d'un nouveau genre vers la côte Sud-Africaine, poussé par le Courant Indien, jusqu'à son arrivée à Port-Élisabeth, où il a enrichi le "North End Park" d'un nouvel arbre exotique.

Mais, même dans cette hypothèse, le phénomène observé à Port-Élisabeth n'aurait pas un moindre intérêt. L'île de Madagascar y gagnerait de pouvoir être considérée comme une grande "escale," établie par le Dieu Créateur et Ordonnateur des Mondes, pour le service des "Messageries maritimes" de la Nature entre les Archipels de la Malaisie et la côte Sud-Africaine.

Veuillez agréer, Monsieur le Rédacteur, les respectueuses salutations de votre humble serviteur,

PAUL CAMBOUÉ, S.J.,
Missionnaire apostolique à Tananarive.

Tananarive, Madagascar, 15 Octobre.

A Marine Millipede.

BRITISH naturalists, especially such as work on the south coast, will hear with interest that Mr. J. Sinel has lately found in Jersey the very curious *marine* Millipede, *Geophilus submaritima*, Grube (*Verh. d. schles. Gesellsch.*, 1872). Dr. Latzel, of Vienna, tells me that the specimens differ somewhat from the type, and probably constitute a well-marked variety. Some examples were found close to the low-water mark of very low spring tides, where they could not be exposed more than two days in a fortnight.

The Geophilus occurs associated with two or three beetles, of which at least one appears to be new, and with a remarkable Chelifer which is probably identical with *Obisium littorale*, a new species described by Moniez from Boulogne, in this month's *Revue Biologique*, or with the doubtful species *O. maritimum* of Leach (*Zool. Miscellany*, iii. 1817).

Mr. Sinel's crowbar, a tool the naturalist makes too little use of, is doing wonderful service. D. W. T.

December 2.

A Case of Chemical Equilibrium.

DURING some experiments made in connection with a research recently laid before the Royal Society, we came upon an interesting case of chemical equilibrium.

The object of the research was to determine the rate of evolution of oxidizing material liberated, under varied conditions, in a solution containing dilute hydrogen chloride and potassium chlorate. There was also introduced a little starch solution and a small quantity of potassium iodide to serve as an indicator of the completion of a certain amount of work, which was the conversion of a known small weight of sodium thiosulphate into tetrathionate. The completion of this change was marked by the appearance of a blue colour in the liquid. The operation was then repeated.

In these experiments the amount of substances undergoing change, when compared with the total amount present, was so large that the masses of the substances remained practically constant during each experiment.

In such a mixture the condition of equilibrium may be considered to be represented by the following equation :

$$n\mathrm{HCl} + m\mathrm{KClO_3} = m\mathrm{HClO_3} + m\mathrm{KCl} + (n - m)\mathrm{HCl},$$

where n is greater than m.

We may then regard the oxidizing material as being liberated by the reaction of the $(n - m)$ molecules of hydrogen chloride with the m molecules of hydrogen chlorate so liberated. The presence of the m molecules of potassium chloride will produce its specific effect (in this case acceleration) on the rate of reaction. So that out of the n molecules of hydrogen chloride employed only $n - m$ are actively engaged in liberating oxidizing material, the rest having been employed in saline decomposition. If such be the case, it ought to be possible to obtain a similar rate of oxidation by taking m molecules of hydrogen chlorate instead of potassium chlorate, and then reducing the hydrogen chloride used from n to $(n - m)$ molecules. If we then add the m molecules of potassium chloride we should then be able to build up a system similar to what is obtained in the former case as regards saline equilibrium. The following results were obtained by this method of procedure.

The numbers signify millionth gram molecules per c.c., and the rates, R, denote the number of millionth gram molecules of $\mathrm{ClO_3}$ decomposed per minute in each cc.

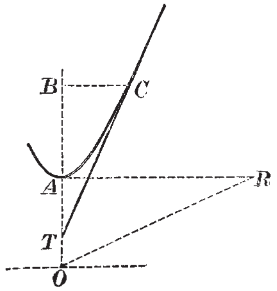
I.	$n = 18 \times 65{\cdot}11$ $m = 6 \times 51{\cdot}5$	A. $n\mathrm{HCl} + m\mathrm{KClO_3}$ gives $R = 0{\cdot}0104$ B. $(n - m)\mathrm{HCl} + m\mathrm{HClO_3} + m\mathrm{KCl}$ gives $R = 0{\cdot}0105$
II.	$n = 15 \times 65{\cdot}11$ $m = 6 \times 51{\cdot}5$	A. $n\mathrm{HCl} + m\mathrm{KClO_3}$ gives $R = 0{\cdot}00554$ B. $(n - m)\mathrm{HCl} + m\mathrm{HClO_3} + m\mathrm{KCl}$ gives $R = 0{\cdot}00555$
III.	$n = 15 \times 65{\cdot}11$ $m = 2 \times 51{\cdot}5$	A. $n\mathrm{HCl} + m\mathrm{KClO_3}$ gives $R = 0{\cdot}00195$ B. $(n - m)\mathrm{HCl} + m\mathrm{HClO_3} + m\mathrm{KCl}$ gives $R = 0{\cdot}00191$

Dover College. W. H. PENDLEBURY.

On the Use of the Word Antiparallel.

AFTER reading Mr. James's note, I looked out the reference quoted by him from Stone's Dictionary in the "Acta Eruditorum." Stone's reference is quite correct, and, as the passage is an interesting one, it may be well to quote it in full. It occurs in an article by Leibnitz treating of the catenary.

"Tangentem ducere ad punctum lineæ datum C ; in AR horizontali per verticem A sumatur R ut fiat OR æqualis OB datæ et ipsi OR ducta *antiparallela* CT, occurrens axi AO in T, erit tangens quæsita.



"*Antiparallelas* compendii causa hic voco ipsas OR et TC si ad parallelas AR et BC faciant non quidem eosdem angulos sed tamen, complemento sibi existentes ad rectum, ARO et BCT."

The following quotation is given in Murray's "New English

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